SZEGEDI ORVOSTUDOMÁNYI EGYETEM
MIKROBIOLÓGIAI INTÉZETE
Igazgató: Dr. IVÁNOVICS GYÖRGY egyetemi tanár

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Prof. JOSHUA LEDERBERG
Department of Genetics
University of Wisconsin
Madison, Wisconsin, U.S.A,

Szeged, 21 August, 1956

Dear Professor Lederberg,

Hoping, you will not mind that I am requesting you to read and criticize one of my manuscript, I shall send a copy of it under separate cover. The article is dealing with a mutation of Bacillus megaterium and it is a continuation of my earlier observations already published. /G. Iványovics, Dissociation of B. megaterium associated with the change of the cell wall antigenic structure, Acta Microbiol. Acad. Sci. Hung., 3, 135, 1955/

As you can see from my first paper on this subject, I ventured to class this mutation as S-R variation. My most recent observations are adding further facts to our knowledge concerning this mutation. No doubt, this mutation can be very well definied on the ground of change in surface antigen /cell wall antigen/ as well as by the resistency to phages developing during the dissociation.

There are, however, some points which might be argued:
1. Am I justified to class this mutation as S-R variation? In fact, S-R variation is based upon the appearance of colonies but later this variation gained a more general meaning than only the colony formation.

2. If we accept that the appearance of colonies is the only sign which justifies us to term a change of characteristics as S-R variation, what sould be our attitude in the case when the formation of capsule is quite indipendent from the deep genetical change of surface material of organism. It is true that the appearance of colonies of Bacillus megaterium is highly influenced by environmental factors, therefore the "rough" or "smooth" type of colony is only a phenotypic expresson of D-glutaminic acid polypeptide production. One can, therefore suppose that the genetical change /mutation/ of cell wall itself can not be accepted as S-R variation for it is indipendent of the roughness or smoothness of colonies.

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3.On the contrary to above mantioned speculation,we can consider the mutation of Bacillus megaterium as S-R variation if we accept the latter as a general phenomenon among microbes which as a rule,is associated with change of cell surface.I am inclined to dwell upon this latter idea although I realize that this opinion might be open to criticizm.

4.I want to add to these that the phage resistency of Bacillus megaterium is not limited to the only strain /207 mutant/ dealt with in manuscript.A number of mutants dereiving from different "wild" strains are behaving same way.Most significant is,however,that I succeeded in isolating also a phage resistent mutant from a "mutilate" strain of Bacillus megaterium.The highly phage sensitive "mutilate" strain used in my experiments was isolated by den DOOREN de JONG /1931/ and the subculture I studied came from Prof.Twoff's laboratory.The mutant obtained from "mutilate" has an identical antigenic structure with 207 mutant strain in spite of the fact that the antigenic structure of wild 207 and "mutilate" strains are not overlapping.I want to stress that I isolated the phage resistant mutants without using any screening effect of phages.

As being the bacterial genetic a new branch of science the conventions of it are still not on sound grounds.I hate to cause any confusion by taxonomy,therefore I wonder should I designate the mutation of Bacillus megaterium as a S-R variation,if I should not what term would be most suitable to denote this particular mutation.That is.why I am appealing to your criticizm.

Will you,please,to inform me about your opinion in this question at your earliest convenience.

Yours sincerely

Prof.George Ivánovics